



JEAN SENEBIER  
1742-1808

## BRIEF PAPERS

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(WITH ONE PLATE)

“Mais la meilleure de toutes les méthodes pour assurer la bonté de ses observations; c'est celle par laquelle on cherche à les appuyer sur de nouvelles observations faites en sens contraire: l'opposition des résultats démontre alors la justesse des premières observations.”

*L'Art d'Observer*, I, 209.

CALANDRINI (1743) first expressed the surmise that the leaves of plants possessed the function of collecting and absorbing dew. This induced a wealthy patrician in Geneva, CHARLES BONNET (1720-1793), to immerse shoots of a grape vine in large glass vessels. He at once observed that these shoots would be covered with innumerable air bubbles as long as sunlight lasted. After sunset, the phenomenon ceased.

This observation, which dates from 1747, opened the door to thousands of experiments at the hand of BONNET's younger contemporaries: PRIESTLEY, INGEN-HOUZ, SENEBIER, and DE SAUSSURE. By cumulated efforts these four investigators, aided by the analytical work of LAVOISIER, succeeded in disclosing the essentials of that mystery which a later period named the phenomenon of photosynthesis, or the discovery of carbonic acid as the chief element of plant nutrition.

By association and inclination SENEBIER was predisposed to a studious life. The son of a wealthy merchant, he was born in Geneva, in 1742 and might have entered commerce, but preferred a free activity as a student of science. His family consented, except for the stipulation that the young man must take up a definite study and finish it. He chose theology and was admitted to the pastorate after three years. At an early age he had been attracted to the circle inspired and guided by BONNET, who fired his younger contemporaries by that curiosity which led to experiments rather than contemplation and philosophy.

After his graduation SENEBIER made a journey to Paris where, on BONNET's advice, he competed for a prize announced by the Haarlem academy for the best answer to the question “Wherein consists the art of making observations?” He won the prize, and returned to Switzerland where, in 1769, he became pastor at Chancy and remained in the pastorate until 1783, when he was called to Geneva as librarian of the City Library. Several works undertaken by him at this period indicate that his inclinations were divided between pure science and literary history. He translated into French the *Opuscula* of SPALLANZANI; he published a literary history of Geneva and calendared and annotated the manuscripts of the municipal library with great zeal. In 1787, he joined the staff of the

renowned *Encyclopédie méthodique* as collaborator for plant physiology. His personal work in this field was published before the Revolution, when SENEBIER returned to the little town of Rolle, carrying his laboratory equipment with him. He continued his physiological, meteorological and chemical researches there for more than ten years and printed numerous papers in the publications of several learned societies in which he held membership, notably Paris, Turin, Geneva and Lausanne. Returning to Geneva in 1799 he divided his attention between the preparation of a translation of the apocryphal books and his *Physiologie végétale* (1800) and died in 1808.

SENEBIER's "physical" experiments, historically an inspiring episode in our knowledge of photosynthesis, began as early as 1765, and their progress, which we shall try to follow, went hand in hand with theoretical and methodological contemplation of the highest order. The results of these studies constitute SENEBIER's first important book: *Essai sur l'art d'observer*, 1775. This book, I believe, is the first systematic attempt at a philosophy of the art of experimentation. The young physicist describes in great detail the mental processes, the points of view, the conscientious attention, and the skill and inventiveness necessary to an experimental worker. Each human quality fully developed, each instance of high inspiration, affords some possibility which insures good and reliable work. In its main thesis, in its appeal to the harmonic personality of the scientific worker, the book still is valid and may be read with advantage even now.

The publications of SENEBIER on plant physiology are quite numerous, but nearly all his experimental work was concentrated in the following books: 1. *Mémoires physico-chimiques sur l'influence de la lumière pour modifier les êtres des trois règnes de la nature et surtout ceux du règne végétal*. 3 vols. Geneva, 1782. 2. *Recherches sur l'influence de la lumière solaire pour métamorphoser l'air fixe en air pur par la végétation*. Geneva, 1783. 3. *Expériences sur l'action de la lumière solaire dans la végétation*. Geneva, 1788. 4. *Physiologie végétale*. 5 vols. Geneva, 1800.

The titles of these works elucidate not only the contents, but, chronologically considered, also the historical development of SENEBIER's studies of the physical basis of photosynthesis. Scarcely any other similar series of experiments in any field of research contains a similar wealth of detail; hundreds and hundreds of times was the same arrangement repeated, now with reference to INGEN-HOUSZ, then testing the exactness of PRIESTLEY's work, then again striking a new track.

The fundamental apparatus was similar to that of INGEN-HOUSZ. The objects (leaves of plants) were deposited in a jar of water; above them and immersed in the jar, stood an inverted funnel, the neck of which was closed at its upper end and graded in order that the gas discharged by the vegetable matter might be measured. But one leaf was used in each experiment, and in each instance its surface was measured by means of a "phyllo-

meter," two glass plates, one ruled to squares, between which each leaf was measured by the number of squares which it covered. The "exhaled" gas was tested by means of VOLTA's eudiometer.

In his first book (1779) SENEBIER denied that his plants ever developed carbonic acid under any circumstances. Green plants developed no gas in the dark. He also pointed out that oxygen was developed only by green plants in sunlight; etiolated leaves, flowers and similar structures gave only negative results. The oxygen originated in the chlorophyll-bearing parenchyma, not from leaf ribs or from epidermal structures. Even small fragments of green parenchyma secreted oxygen.

Another series of experiments was concerned with the influence of colored light on the formation of oxygen. The priority of these studies is his. He employed large, double-walled bell-bottles and tested his objects with blue (litmus solution) and yellow (curcuma extract) light. The results indicated abnormal relations, but these were considered due to the amount of light, not to its quality.

The most important of all SENEBIER's discoveries was, however, that the presence of carbonic acid proved a deciding factor in the development of oxygen by green plants. His theory of this phenomenon was that the carbonic acid was dissociated and thus the oxygen liberated. This was proved by three important facts: a, In distilled water, leaves developed no oxygen (already pointed out by BONNET (1754); b, the amount of oxygen rose and fell with the amount of carbonic acid in the water within certain limits; c, carbonate of lime gave off no oxygen in distilled water, but if some acid was added which liberated the carbonic acid in this water, green leaves, when introduced and exposed to sunlight, would develop oxygen.

These data constitute SENEBIER's chief contribution to the solution of the problem. He did not, however, stop at this point. He assured himself that the gas which was dissociated by the action of green leaves was the so-called "fixed gas" (carbonic acid), the amount of which, in the water, decreased considerably as the leaves acted upon it. The leaves separated the useless from the useful admixture, expelled the "pure air," *i.e.*, the oxygen, and absorbed the phlogiston as an element in their development and growth. He further concluded that the metabolism of plants consisted in the association of carbonic acid, plant juices and light, this process being confined to green tissues, probably to their resinous elements (*i.e.*, the chlorophyll). The juices of plants were replenished through the roots, which absorbed water, some solid matter, and carbonic acid (dissolved in the water).

The source of the exhaled oxygen was an important problem, and SENEBIER at once asked whether it might be attributed to atmospheric carbonic acid. His reasoning on this point is very interesting: "Carbonic acid is, and always must be, present in the lower strata of the atmosphere . . .

the problem is whether it may enter into the leaves. I confess that I do not believe carbonic acid can be carried into the leaves as a gas . . . but that it enters after having been dissolved in the water, like in charged waters."

His next conclusion was that the carbonic acid, thus present in the plant tissues, was decomposed by sunlight; the phlogiston (carbon) united with the "resinous substances" (chlorophyll), which have a great affinity for carbon.

His most important conclusion, however, was: "If the amount of oxygen given off by the leaves, is proportionate to the amount of carbonic acid in the water, and if the leaves in the water have absorbed only the carbonic acid contained therein, then the gas which is produced must be the result of a dissociation of the carbonic acid."

Scarcely less elucidating was his next conclusion, that the service of the carbonic acid lies in assisting in the formation of the acid substances contained in the plant.

Thus, in 1788, SENEBIER recognized—naturally on the basis of LAVOISIER'S analysis of carbonic acid—that "phlogiston" was identical with carbon; that carbonic acid is dissociated within the plant tissues; and finally, that the plants serve as regulators of the atmospheric content of carbonic acid.

In 1792 this subject was resumed in a paper in the *Journal de Physique*.

In this paper he confirmed his former findings with the following addition: "In regard to the formation of the amount of hydrogen necessary for the production of oils and vegetable acids, this is doubtless due to the dissociation of the water, but experience has not yet taught me how it takes place in the plants." This shows that SENEBIER at this time considered water not only a medium of solution from which plants might extract certain gases dissolved therein, but as a nutritive element, subject, in itself, of an advantageous dissociation.

Substantially the same conclusions were deposited in the *Physiologie végétale*, 1800.

As is well known, INGEN-HOUSZ never recognized the fact that the secretion of oxygen depends on the presence of  $\text{CO}_2$  in the plant. The controversy between these two investigators, at times very bitter, was continued for many years. The details, embodied in more or less tart papers, cannot now interest us deeply. To INGEN-HOUSZ belongs the credit of explaining PRIESTLEY'S discovery that living green plants "improve" the atmosphere in such a way that it will increase combustion and sustain the life of animals. This was abundantly (and independently) confirmed by SENEBIER. Neither had perfected his experiments far enough to prove conclusively that  $\text{CO}_2$  is developed by plants in the absence of light. The share of each of these two men in the discovery of almost identical facts has been subject of much discussion and even controversy; the fact remains that INGEN-

Housz admitted the development of oxygen by chlorophyll-bearing plants without the presence of  $\text{CO}_2$ . But neither *proved* the assimilation of  $\text{CO}_2$  from the atmosphere, even though INGEN-HOUSZ observed and recognized it. Either, however, *carried Priestley's and Bonnet's observations into the field of exact experimentation*, and but few fundamental discoveries in plant physiology have been as carefully and persistently documented.

SENEBIER's studies of the cause of etiolation were quite exhaustive, but barren of valid result. More successful were his studies on the sleep-movements (afterwards continued by his friend A. DE CANDOLLE), by which it was proved that a degree of turgor remains even amidst the periodic movements.—J. CHRISTIAN BAY, *John Crerar Library, Chicago, Illinois*.

### GRAFTING EXPERIMENTS WITH COTTON<sup>1</sup>

(WITH ONE FIGURE)

A successful method of reproducing cotton asexually offers interesting possibilities in retaining parental genotypes of this crop indefinitely. In previous papers the author has described his attempts to propagate cotton by stem cutting.<sup>2,3</sup> More recently some preliminary experiments in grafting cotton have been completed and it is desired to present the results of this work in the present paper. While no attempts have been made by the author to try budding in connection with the propagation of cotton, McNAMARA and HOOTON<sup>4</sup> of the U. S. Cotton Breeding Station at Greenville, Texas, have succeeded in propagating cotton by budding. Grafting as a method of reproducing cotton asexually has received the attention of the writer only after poor results were secured with cuttings. Although numerous attempts have been made to root cotton stem cuttings, less than 10 per cent. of the cuttings resulted in new plants. Since little or no difficulty was experienced in securing a high percentage of callusing in stem cuttings, it was expected that grafting might be very successful.

The saddle graft method was used in these trials, fig. 1.<sup>5</sup> The main stem of the cotton plant to be used as the stock was trimmed to a slender wedge immediately above the lower node. Any leaves or branches below this node were removed. A scion of medium mature wood and of similar diameter to the stock was selected and cut to retain three nodes. In preparing the scion a cross-sectional cut was made immediately below a basal bud. The lower end of the scion was then split a short distance and fitted over the wedge of the stock so that the cambium layers of the scion and stock were matched

<sup>1</sup> Contribution from the Division of Agronomy, Texas Agricultural Experiment Station. Approved by the Director as Technical Contribution no. 130.

<sup>2</sup> REA, H. E. Asexual reproduction of cotton. *Jour. Hered.* 19: 356-357. 1928.

<sup>3</sup> REA, H. E. Callusing of cotton stem cuttings. *Plant Physiol.* 5: 575-585. 1930.

<sup>4</sup> McNAMARA, H. C., and HOOTON, D. R. Unpublished data.

<sup>5</sup> BAILEY, L. H. *The nursery book*. Macmillan Co. 1912.